MONTHLY NOTICES

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LORD LINDSAY, M.P., President, in the Chair.

M. Camille Flammarion, Avenue de l'Observatoire 36, Paris; and

Alfred J. P. Shepherd, Esq., B.A., Queen's College, Oxford;

were balloted for and duly elected Fellows of the Society.

The Astronomer Royal communicated to the Council a letter relating to the erection of a statue to Le Verrier, addressed to him by M. Fizeau, President of the Academy of Sciences, and of a committee formed for promoting that object. The fund is to be raised by voluntary subscriptions. The committee includes already the President, the Vice-Presidents, and a large number of the Members of the Academy. Letters may be addressed to the Secretariat of the Institute of France, where a register of subscriptions has been opened.

The Total Solar Eclipse of 1878, *July* 29.

The following Notice to intending observers of the Total Solar Eclipse of 1878, July 29, has been issued by the Superintendent of the United States Naval Observatory:—

It having been suggested that various men of science in Europe desire to observe this eclipse, and that it might be possible, in some cases, to obtain for them reduced rates of fare on the railway lines of the United States, the case was presented to the Pennsylvania Railroad Company, and the following arrangements for a reduced rate of fare to and from the East to Denver, Colorado (which is near the Central line), have been

offered by the Company. It will be noticed that these arrangements refer only to persons coming in a private capacity, and not to members of expeditions sent out by foreign governments.

[Extract.]

PHILADELPHIA, *January* 7, 1878.

SAMUEL M. FELTON, *Philadelphia.*

DEAR SIR,—Your letter of the third ultimo, addressed to THOMAS A. SCOTT, President, with enclosure from Professor EDWARD S. HOLDEN, U. S. Naval Observatory, Washington, requesting reduced rates from the East to Denver, Colorado, and return, for such scientific gentlemen from Europe as may wish to observe the total solar eclipse of 1878, July 29, has been referred to me for attention, and, in reply, I beg to state that the Pennsylvania Railroad Company is anxious to afford all possible facilities to persons coming in a private capacity for this purpose.

Upon the order of the Superintendent of the U. S. Naval Observatory, Washington, I will furnish transportation to Denver and return *viâ* Pittsburg, Indianapolis, St. Louis, and Kansas City, or *viâ* Pittsburg, Chicago, and Omaha, at the following rates for the round trip: From New York, $73.00; from Philadelphia, $71.00; from Baltimore or Washington, $69.50. . . . . .

L. P. FARMER,
General Passenger Agent.

The round trip ticket includes transportation (about 2,000 miles according to route) from New York to Denver and back again to point of starting; or in all 4,000 miles of travel.

These arrangements allow the journey to Denver to be made by one route and the return by another. The routes available are:—

No. 1. *Viâ* Pittsburg, Chicago, Omaha, Denver.
No. 2. *Viâ* Pittsburg, Chicago, Omaha, Denver.
No. 3. *Viâ* Pittsburg, Chicago, Omaha, Denver.

Different routes from Chicago to Omaha.

No. 4. *Viâ* Pittsburg, Chicago, Leavenworth, Denver.
No. 5. *Viâ* Pittsburg, Chicago, Atchison, Pueblo, Denver.
No. 6. *Viâ* Pittsburg, Indianapolis, St. Louis, Kansas City, Denver.
No. 7. *Viâ* Pittsburg, Indianapolis, St. Louis, Kansas City, Denver.

Different routes from St. Louis to Kansas City.

No. 8. *Viâ* Pittsburg, Indianapolis, St. Louis, Kansas City, Pueblo, Denver.
No. 9. *Viâ* Pittsburg, Indianapolis, St. Louis, Kansas City, Pueblo, Denver.

Different routes from Kansas City to Pueblo.

Persons intending to make observations of this eclipse, and desiring to take advantage of the courtesy extended by the Pennsylvania Railroad Company to foreign men of science (by which the ordinary cost of transportation is reduced one-half), will please notify by letter the Superintendent of the U. S. Naval Observatory of their intention, and request that a letter certifying to their identity be forwarded to their Consuls at the port they may select for arrival in the United States. This

letter must, upon delivery, be countersigned by the Consul of the applicant to prevent mistakes.

The Hon. Secretary of the Treasury has notified the collector of customs of the ports of Boston, Portland, New York, Philadelphia, and Baltimore of the probable arrival of European observers of this eclipse, who will be identified by exhibiting the above letter, so signed and countersigned, to the collectors named. Orders have been issued to them as follows :—

"Upon the arrival at your port of any of the gentlemen mentioned, you will extend all proper facilities for the speedy delivery to them of the professional instruments in question free of duty and charges."

On presenting the same letter, so countersigned, to the agent of the Pennsylvania Railroad Company in New York, Philadelphia, Baltimore, or Washington, a round-trip ticket to Denver and return will be issued to them by the Railroad Company at the rates before named.

From Denver railroad lines extend to points likely to be chosen for observing stations.

JOHN RODGERS.
Rear Admiral, U. S. Navy, Superintendent.

U. S. NAVAL OBSERVATORY,
Washington, February 1878.

The Luminous Spot on Mercury in Transit.

By B. G. Jenkins, Esq.

The attention of the scientific world will be directed on the 6th of May next to an astronomical occurrence of more than ordinary importance—a May transit of the planet *Mercury*, which has not occurred since the year 1845, although three November transits have taken place within the last thirty years.

Within the last 170 years there have been thirteen transits, upon which some observations on the luminous appendages of the planet have been made; but to come nearer to our own time, within the last forty-five years there have been in all six—on May 5, 1832; November 7, 1835; May 8, 1845; November 8, 1848; November 11, 1861; and November 4, 1868. It will be observed that four of these occurred in November, and only two in May. This is important, because the planet is in two opposite points of its orbit in these months—in November being only about five days from perihelion, and in May about ten days from aphelion, in which position it will be on the 6th of May next.

As we have not had a May transit since 1845, when it was only partly visible in this country, and the previous one in 1832 was unsatisfactorily observed, on account of the unfavourable weather, the coming transit will be of more than ordinary

importance. It will be well in examining the transits to divide them into May transits and November transits, and, having compared them, to see what conclusions can be drawn in regard to the luminous appendages.

In the transit of 1799, which occurred in May, Schröter and Harding at Lilienthal, and Köhler at Dresden, saw a small luminous spot on the dark disk. The spot was not stationary, for Harding saw it change its position, and later in the day Schröter saw it sometimes on one part of the disk, sometimes on another. Other observers saw, not one, but two small spots of a greyish colour.

In the transit of May 1832 Professor Moll observed a spot, the periphery of which was not well defined, but was always situated in the same position, a little south of the centre of the planet, and preceding the centre.

Turning now to the November transits for further evidence of the luminous spot, we find that the transit of 1835 was not visible in this part of the world, and in the three previous ones of which we have some particulars, viz. those of 1736, 1789, and 1802, no mention is made of a luminous spot; but, as will be seen, it does not follow that it did not exist because it was not observed.

In November 1848 one spot was observed of a greyish colour, shading off on all sides from the centre.

In November 1861 Mr. J. W. Jeans saw a slight ashy light on the eastern limit or following the centre of the planet on its way to perihelion.

In November 1868 Mr. Huggins saw a point of light nearly in the centre which had no sensible diameter, but appeared as a luminous point. It was near the centre, a little south, and following the centre. It remained in the same position during the transit.

Mr. Browning, observing this transit, saw near the centre of the planet two bright grey spots close together, both a little south of the centre, one more conspicuous than the other.

So much for the luminous spot. Let us turn to the rings which have been observed round the planet during a transit.

In May 1707 the late Assistant of the then Astronomer Royal saw *Mercury* in transit encompassed by a thick haze or atmosphere.

In the transit of May 1753 a ring was observed round the planet; also in that of May 1786.

In May 1799 a dark or nebulous ring was observed, the tinge being of a violet hue and strongest near the planet. Schröter also saw the ring, the light of which he says was scarcely brighter than the surface of the Sun, but of another colour. Harding did not see it at first, but he did afterwards when he used a lighter shade.

In May 1832 the planet was seen at the Royal Observatory surrounded by a dusky tinge. In this transit also the ring has been described as of "a violet hue, the colour being strongest near the planet."

This is the sum of our knowledge about the rings round the planet in the May transits, and from it the natural inference appears to be that when the planet is in transit near its furthest distance from the Sun, it is generally surrounded by a dark nebulous ring.

The first November transit in which we have mention of a ring is that of 1736. Professor Moll says Plantade saw the reverse of what he saw in May 1832 ; so that the ring must have been a bright one.

In 1789 and 1802 rings—probably bright ones—were observed. The transit of 1835 was not visible in Europe ; that of 1848 was, however, and many observers saw a bright ring, although Mr. Breen says he did not see any.

In November 1868 Mr. Huggins saw the planet in transit surrounded by an aureola of light, a little brighter than that of the Sun's disk.

Here the weight of evidence appears to be in favour of a bright ring round the planet when near the Sun, which would probably be the case. It should be stated, however, that Prosperin calls the ring of May 1786 a luminous one, and Ljungberg that of November 1802 a dark one ; but it must be remembered that the ring of the transit in 1799, which there can be no doubt was dark, nebulous, and of a violet hue, is described by Schröter as scarcely brighter than the surface of the Sun, and of another colour.

Collecting these results and comparing them, it will, I believe, be found :

1st. That in the May transits, when *Mercury* is near its aphelion, the luminous spot is in advance of the planet, preceding the centre ; in the November transits, when *Mercury* is near its perihelion, the luminous spot follows the planet.

2nd. The luminous spot has never been seen at the centre, but always south of it, and therefore cannot be due to diffraction, as Professor Powell suggests.

3rd. Sometimes in the same transit two spots have been seen close together, where shortly before only one was observed.

4th. In the May transits the rings round the planet were dark or nebulous and of a violet tinge ; in the November transits they were bright.

5th. If we take the two transits which have received the most careful observation, the May transit in 1832, observed by Professor Moll, and the November transit in 1868, observed by Dr. Huggins, we find the contrast very great and very typical : in the one case a diffused spot preceding the centre, with dark ring surrounding the planet, in the other a sharply defined spot following the centre, with bright ring surrounding the planet.

6th. From all which I conclude that on May 6 next, when *Mercury* will be at almost its furthest distance from the Sun, the luminous spot will not be a point of light having no sensible diameter, but rather diffused, with a periphery not well defined,

gradually sinking from a greyish white to the dark colour of the disk, situated a little in advance of the centre and to the south of it, and that the planet will be surrounded by a dark nebulous ring, not a bright one.

4 *Buccleuch Road, Dulwich,*
March 25.

Early Transits of Mercury. By the Rev. S. J. Johnson.

On occasion of the transit of *Mercury* in 1861 I found the planet distinctly perceptible on the Sun with an old ship glass carrying a power of 16. Between the introduction of the telescope in the year 1610 and Gassendi's observation in 1631 three transits of *Mercury* took place, each of which was more or less visible to Europe. These were

1615	May 3	Ingress about $6\frac{1}{2}^h$ morn.	Egress about $1\frac{1}{2}^h$ aftern.
1618	Nov. 4	„ „ $11\frac{1}{2}$ morn.	„
1628	May 5	„ „ $2\frac{1}{4}$ aftern.	„

As many telescopes at this period were probably twice as powerful as the glass I have referred to, is it not possible there may be diagrams of Sun-spots extant showing the little round speck of *Mercury*? Galileo's *Macchie Solari* gives several solar spot diagrams for 1613. Scheiner's *Rosa Ursina* contains several for the year 1625, but there are no doubt other works giving the like.

A year before Horrox obtained a successful observation of a transit of *Venus* he looked unsuccessfully for one of *Mercury*. As this is not referred to in any popular modern work, his words may be given:—

"1638, Octob. 20.—Paulo post ortum solis vidi, per telescopium maculas duas in sole, easdem vidi meridie et diebus sequentibus. At *Mercurium* non vidi. Ex calculo Lausbergii fuisset *Mercurius* in centro Solis horâ 5 manè.

Octob. 21.—Manè. Nullum vidi *Mercurium* in sole, ergo *Mercurius* vel ob latitudinem non ivit sub Solem, vel fuit conjunctio nocte præcedente: flavit enim ventus insolitâ violentiâ. Calculus Rudolphinus facit conjunctionem horâ 11·47′ quâ ego non observavi. Observatio autem Gassendi A.C. 1631 Octob. 28 probat conjunctionem fuisse citiùs per horas 4·43. Ergo vera conjunctio fuisset horâ 7·4′ paulo ante ortum Solis: at statim post ortum Solis nihil visum. Ergo latitudo *Mercurii* major erat semidiametro Solis. Quod etiam probat Gassendi observatio." (*Catalogus observationum Jeremiæ Horroccii*, 1672).

Horrox was at Toxteth at the time, before he went to Hoole. His failure does not appear to have been from insufficient telescopic

power. On making an approximate calculation of the places of *Mercury* and the Sun by the same method as for the future transits of the planet (*Monthly Notices*, May 1877) I found *Mercury* slightly escaped a passage across the Sun's disk on this occasion.

Upton Helions Rectory, Crediton,
April 9, 1878.

A Comparison of the Observations of Contact of Venus with the Sun's limb in the Transit of 1874, *December* 8, *made at the Royal Observatory, Cape of Good Hope, with the corresponding observations made at the stations in the Parliamentary Report, and a discussion of the results.* By E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer, Royal Observatory, Cape of Good Hope.

The Cape observations of the times of contact of *Venus* with the Sun's limb at Egress on 1874, December 8, were immediately after the transit forwarded to Greenwich to be worked up with observations of contact made at the other stations. The Cape observations were not, however, included in the Parliamentary Report, and I should not have felt myself at liberty to anticipate the official publication of the results were it not that some values of the solar parallax, supposed to have been derived from their use, have been published in the *Observatory* Magazine for 1878, February. The value of the solar parallax which is given in that magazine, as the result of a comparison between the Cape observations of the phase ζ, or what I call the "real contact," and the corresponding observations at six stations where the contact was retarded at Egress, is $8''{\cdot}495$. I feel certain that no such value of the solar parallax can be fairly derived from our observations, and I have taken some trouble to test this matter by direct computation and comparison of the results. At the time of the transit there were only two instruments at the Cape fit for the delicate work of observing contacts for a determination of a value of the solar parallax. I observed the contacts with the 7-inch telescope by Merz and employed a power of about 200; Mr. Finlay, my first assistant, used a good 4-inch telescope by Dollond and employed a power of about 180. The contact was also observed by Mr. G. Maclear with a small theodolite of which the object glass had an aperture of 1·65 inches. He employed a power of only 23. With such optical means it was of course impossible for Mr. Maclear to attempt to discriminate between changes of the Sun's limb which are presented near the point of contact, and his observation is not available for comparison with contacts made with larger instruments and with greater optical power. But I thought that an observation made by a careful

observer with small optical power might be useful in discussions respecting the nature of the contacts observed.

The following are the contacts observed:—

1874, Dec. 8.

Observer, E. J. Stone.

	Cape Mean Times. h	m	s	
(1)	19	6	25	Slight connection, but not firmly established; good.
(2)	19	7	12	Continuous now; a fair breadth.
(3)	19	8	5	Apparent contact.

At the apparent contact a considerable portion of the Sun's limb was cut out, or the ligament was about one-third of the diameter of *Venus*.

Observer, W. H. Finlay.

	h	m	s	
(2)	19	7	10·7	Permanent connection seen.
(3)	19	8	18·3	Apparent contact; considered rather late.

The time, $19^h\ 8^m\ 18^s{\cdot}3$, has been increased 30 seconds for error of count by Mr. Black. Mr. Black noticed at the time of making the entry that he was then counting from the wrong end of the seconds hand. A note of this and of the effect of the error was made at the time, and before any comparison had been made with other observations.

Observer, G. W. H. Maclear.

h	m	s	
19	7	48	First permanent contact.

There can be no doubt about the selection of the contacts which, at the Cape, correspond to the phase ζ. If we take the mean of my time, $19^h\ 7^m\ 12^s$, and Mr. Finlay's, $19^h\ 7^m\ 10^s{\cdot}7$, we have $19^h\ 7^m\ 11^s{\cdot}35$ Cape mean time for the observed phase.

The following table is extracted from the *Observatory*, 1878, February.

Results from Egress—Phase ζ.

Stations.	Acceleration. s	Parallax. ″	No. obs.	Weight.
Sydney (N.S.W.)	388	8·768	5, 11	77
Goulburn (N.S.W.)	388	8·891	2, 11	54
Woodford (N.S.W.)	382	8·801	1, 11	36
Victoria	406	8·900	3, 11	64
Adelaide	339	8·778	3, 11	57
Cape Observatory	187	8·495	2, 11	30
Port Elizabeth (C.G.H.)	165	8·385	1, 11	21

Stations.	Retardation. s	Parallax. ″	No. Obs.	Weight.
Mokattam and Suez	594	8·774	4, 17	96
Luxor (Egypt)	562	8·780	3, 17	78
Abbasseyah (Cairo)	594	8·780	1, 17	45
Roorkee (India)	544	8·804	1, 17	45
Madras (India)	426	8·918	1, 17	31
Mussoorie (India)	563	8·597	1, 17	45

I have not all the materials which have been used in the determination of the value 8″·495 placed opposite the Cape Observatory; but, as the Cape observations are only compared with 11 observations of Egress retarded, and as the mean value assigned to the last 4ˢ scarcely differs from the mean value found from the first 7, if the value 8″·495 be correct some such value must be found from a comparison with the 7 contacts observed at Mokattam and Suez, and Thebes which are printed in the Parliamentary Report.

I have therefore computed the tabular distances between the centres for my own station and for the stations Mokattam and Suez, and Thebes. The value of the solar parallax adopted is 8″·90. The following are my results :—

	′	″		
Mokattam and Suez	15	41·679	$-2\cdot257\,\delta\pi = C$	4 obs.
Thebes	15	41·783	$-2\cdot134\,\delta\pi = C$	3 obs.
Cape	15	41·879	$+0\cdot616\,\delta\pi = C$	2 obs.

The agreement between these results is so close that really no sensible correction to the assumed value of the solar parallax would appear necessary. However, if we combine the Cape and Suez we find

$$\delta\pi = -\frac{0\cdot200}{2\cdot873} = -0\cdot069, \quad \text{or} \quad \pi = 8''\cdot831;$$

and if we combine the Cape and Thebes, we find

$$\delta\pi = -\frac{0\cdot096}{2\cdot750} = -0\cdot035, \quad \text{or} \quad \pi = 8''\cdot865;$$

whence, in the mean,

$$\pi = 8''\cdot846 \pm 0\cdot22\ \epsilon.$$

The value of the solar parallax thus deduced is well within the probable error, if the true value of the solar parallax be as large as 8″·90.

I am at a loss to understand how the value 8″·495 can have been obtained from the Cape observations, unless the time of Mr. Maclear's contact, $19^h\ 17^m\ 48^s$, has been virtually adopted

as the time of contact, and a second contact agreeing with this has been obtained by taking Mr. Finlay's apparent contact for a real contact, and neglecting to apply the 30 seconds required for error in count, or by erroneously correcting his time for phase ζ by 30 seconds. But in these cases all my observations must have been deliberately rejected, and Mr. Finlay's unduly dealt with. I can hardly believe that such steps can have been taken in an important investigation, but I can see no other explanation which will account for the value of the solar parallax deduced from the Cape observations.

I will now compare all the three kinds of contact which I observed at the Cape with the corresponding observations given in the Parliamentary Report. The adopted value of the mean solar parallax is $8''{\cdot}95$ which is the value of the Parliamentary Report.

First Phase, or δ.

We have five observations available, four from the Parliamentary Report and my observation at the Cape. The corresponding distances between the centres are

		′	″
	Mr. Hunter, Suez	15	40·08
	Mrs. Campbell, Thebes	15	39·31
(1)	Lieut. Hoggan, Rodriguez	15	40·30
	Lieut. Goodridge, Kerguelen	15	40·61
	Mr. Stone, Cape	15	40·38
	Mean	15	40·14

The Cape observation of this early phase therefore agrees within $0''{\cdot}24$ with the mean of the five observations.

Second phase or ζ.

We have for the distances between the centres

		′	″	
	From 4 obs. at Suez	15	41·46	
	„ 4 „ Thebes	15	41·37	
(2)	„ 5 „ Rodriguez	15	41·43	
	„ 2 „ Cape	15	41·79	Stone & Finlay
	„ 3 „ Kerguelen	15	41·28	
	The mean, with weights, is	15	41·44	

The Cape result therefore only differs $0''{\cdot}35$ from the mean. I have taken for Kerguelen the mean of the three observations by Perry, Sidgreaves, and Goodridge.

Third phase or η.

We have the following observations of apparent contact:

			′	″	
	2 obs. at Suez		15	44·54	
(3)	3 „	Rodriguez	15	43·31	
	2 „	Cape	15	43·61	Stone & Finlay
		The mean is	15	43·75	

and the Cape observation differs 0″·14 from the mean.

I have given these comparisons and called attention to the close agreement between the Cape results and the mean results for all the stations because, when my observations were first sent to England, I was told that I had allowed my theoretical views to influence my observations. The comparisons here given afford my answer to the charges thus brought against me, and are a proof of the correctness of my theoretical views. An inspection of the results for the phases (1), (2), and (3) will show that no mistake need be made in referring an observation at least to its proper class. But if an observation of phase (1) at one station be combined with an observation of phase (3) at another station, the result must be an erroneous value of the solar parallax to the amount of about 3″ divided by the sum of the parallax factors for the stations under consideration; or, if an observation of what I call real contact, or phase (2), be combined with an apparent contact, or phase (3), the result must be an erroneous value of the solar parallax to about the amount of 2″ divided by the sum of the factors. But if observations of phase (2) be alone combined, the discordance should be confined within comparatively speaking small limits. The method is a strong one, but requires, of course like all other methods, to be correctly applied. The views here expressed are those upon which I acted in my rediscussion of the transit of 1769, and without their substantial adoption no discussion of the observations made in 1874 is possible.

If we attempt to obtain a value of the solar parallax from the few observations of the phases δ and η collected in the Admiralty Report with the addition of only the Cape observations, it must of course be expected that the results should run wild. The probable errors of such results must be considerable, and the theory of probable errors itself is hardly applicable in a discussion of such small groups of observations. I shall, however, exhibit the results partly as a matter of curiosity and partly to show how, by mutual elimination of error, even a few observations rapidly approximate to the true result. The five observations of phase δ lead to the value

$$8''{\cdot}69 \pm 0{\cdot}322\ \epsilon, \quad \text{or} \quad 8''{\cdot}69 \pm 0''{\cdot}161,$$

and the seven apparent contacts to the value

$$9''{\cdot}31 \pm 0{\cdot}331\ \epsilon, \quad \text{or} \quad 9''{\cdot}31 \pm 0''{\cdot}166.$$

If we combine these results, we have

$$\pi = 8''{\cdot}996 \pm 0''{\cdot}116,$$

a result which, by its probable error, touches the value $8''{\cdot}88$.

If we combine the Cape observations of phase ζ with the other observations of the same phase collected in the Parliamentary Report, and use, as in my paper of January, the mean of all the contacts observed at Kerguelen for the phase ζ, we have the following equations:

No. Obs.		Residuals. (Obsd.—Compd.)
4	$x + 0{\cdot}202\ y = 0{\cdot}455$	$+0{\cdot}035$
4	$x + 0{\cdot}191\ y = 0{\cdot}368$	$-0{\cdot}053$
5	$x + 0{\cdot}027\ y = 0{\cdot}426$	$-0{\cdot}017$
2	$x - 0{\cdot}057\ y = 0{\cdot}793$	$+0{\cdot}340$
3	$x - 0{\cdot}100\ y = 0{\cdot}283$	$-0{\cdot}176$

These equations lead to

$$18\ x + 1{\cdot}293\ y = 0{\cdot}7857$$
$$1{\cdot}293\ x + 0{\cdot}3428\ y = 0{\cdot}53096,$$

from which

$$x = 0''{\cdot}446, \qquad y = -0{\cdot}13,$$

or, since $\pi = 8{\cdot}95 \left(1 + \frac{y}{100}\right)$,

we have

$$\pi = 8''{\cdot}938 \pm 0{\cdot}175 \times \epsilon,$$

a result which is, I believe, within its probable error of about $0''{\cdot}053$.

The value which I obtained from the Ingress observations was $8''{\cdot}860 \pm 0{\cdot}136\ \epsilon$.

The result of the introduction of the Cape observations is therefore slightly to diminish the large value derived from the Egress observations when the Kerguelen observations, which were picked up through cloud, were alone available for accelerated Egress. Of course if the Kerguelen observations are not good (I mean good in the sense of being strictly comparable with the observations made at the other stations), then the observations of Egress made in Australia may modify this result; but the value of the solar parallax must, I believe, lie between $8''{\cdot}84$ and $8''{\cdot}92$. The value of the solar parallax at present entitled to the greatest weight appears to me to be

$$8''{\cdot}88 \pm 0''{\cdot}02.$$

1878, *March* 12.

Addendum.

I received yesterday morning copies of the Port Elizabeth observations, the originals of which were forwarded to England in April 1875. There are three times of contact given:

	h	m	s	
(i)	7	25	13	[The correction to reduce these to Cape Mean Time is about $-18^m\ 6^s$.]
(ii)	7	25	23	
(iii)	7	25	38·5	

"When observation (i) was recorded the contact seemed so nearly perfect that only a thin dark line along the edge of the Sun proved the contact hardly consummated.

"When observation (ii) was recorded the appearance of the limb was as though blended in one sharp defining line: hence set down as the moment of perfect contact.

"When observation (iii) was recorded the contact at that moment may be described simply as fully perceptible or certain but with no observable loss of its circular form in any appreciable degree."

The above was the description of contact (i) in the original paper. If that description had been correct these observations would appear to have been observations of apparent contacts, but then a comparison with the Cape observations appeared to show that they must be a minute wrong. I wrote therefore in 1875, asking how long before (i) the dark line was first seen, and whether there was any possibility of a mistake having been made in the minutes. The answer was that the *dark* line was a mistake; the line should have been described as a *bright* line, and that the minutes were thought to be correct. The change of the word *dark* to *bright* of course changed the interpretation to be put on these observations, which must be attempts at real contact, and in this case the minutes are correct. If we take the mean times given for the contacts (ii) and (iii), as we have done at Rodriguez and Kerguelen, the equation of condition for Port Elizabeth is

$$15'\ \ 41''{\cdot}64 + 0{\cdot}564\ \delta\pi = C,$$

and this, combined with those for Thebes and Mokattam and Suez, shows that the value $8''{\cdot}93$ satisfies the Port Elizabeth observation. I can only account for the value $8''{\cdot}385$ assigned to the Port Elizabeth observation in the *Observatory* magazine by supposing that the minutes have been erroneously altered. The only ground for supposing the minutes wrong was the idea at first that the observations were apparent contacts, and this idea is disproved by the bright line seen to separate the limbs only 15^s before the contact. This will be clearly apparent on comparison of the phases δ, ζ, and η given at the different stations.

1878, *March* 12.

Photography at the Least Refrangible End of the Solar Spectrum.
By Capt. Abney, R.E., F.R.S.

Nearly two years ago I had the honour of reading a preliminary note on photographing the least refrangible end of the spectrum, and it seems that the time has come when I ought to redeem the promise implied by a "preliminary note" and enter further into the subject. Last year, owing to a change in residence, I was unable to pursue the subject with any degree of activity, but during this last winter and the present spring I have made fair progress in my researches, the results of which I lay before the meeting. These results are principally photographs themselves, and the first to which I shall call attention is one taken through three prisms of dense flint glass, each of which had a vertical angle of 62°. They were placed at the angle of minimum deviation of B and kept so. The focal lengths of the collimator and camera were 18 inches and 2 feet respectively, and a condensing lens of 6 feet focus was employed to collect the light, the middle of the collimating lens alone being filled with solar rays. In front of the slit was placed a plate of orange glass, in order to cut off the suffused blue rays, which experience had previously taught me were inimical to the production of good negatives, owing to the light dispersed in the prisms themselves. It will be noticed to what an enormous distance below A the impressions of the bands in the ultra red are to be found. If the wave lengths be used as abscissæ and the measured distances of the known lines be used as ordinates, it will be found, if the waves be completed by hand, that a wave length of not less than 10,400 tenth metres is impressed. Roughly speaking, A is 7,600 tenth metres and D 5,900 tenth metres, by which it will be seen that I was within the mark when I announced that I had obtained photographs as much below A as D was above it. Now this negative, though interesting as a feat in photography, has no practical scientific value, as the ultra red is so tremendously compressed that the absolute wave lengths could not be obtained from it.

About the time I read my last paper, Captain Tupman kindly lent me a speculum metal grating, by Rutherford, having about 8,600 lines to the inch; but it was only lately, after removing its glass covering, that I was fully able to appreciate its value. In all gratings the red, or rather the ultra red, of the first order is overlapped by the ultra violet and violet of the second order, and the higher the order the more overlap there is. To remedy this, which was a defect for the purpose for which I required it, I placed before the slit of the collimator red glass, which completely cut off the yellow and only allowed a little of the green to pass. The prisms were replaced by the grating, and a photograph of the second order of the spectrum was taken by it. Theoretically speaking, the time necessary for taking a photograph of the first order is $\frac{1}{8}$ that for taking one of the second, and $\frac{1}{27}$ for that of the third. Practically, this is not quite true, for reasons which it is

unnecessary to enter into here. This induced me to think that if I could get a grating with double the number of lines to the inch, its first order should give me the same dispersion as the second order of the grating I was using, and at the same time the exposure ought to be more than halved. Mr. Lockyer kindly lent me such a grating, and the third photograph was taken by it. You will see that it gives the lines from C to A almost perfectly. A positive copy of this photograph I sent to Professor Piazzi Smyth, as he drew the map of this region of the spectrum which he published in the last volume of the *Edinburgh Astronomical Observations*; and I cannot do better than quote his words regarding the accuracy of the photograph :—"As the size of the glasses approximated closely to my standard camera size, 2·25 inches × 4·25 inches, I was able at once to view them with any magnifying power in a compound microscope long since arranged for such things, and the effect was astounding. I almost thought I was back again in Lisbon, viewing the Sun's spectrum itself as I used to see it."

This acknowledgment of the value of the photograph was particularly gratifying, as it came from an astronomer who had paid particular attention to this particular part of the spectrum.

The next plate shows the region of the ultra red as taken with the same grating. The most conspicuous group has a wave length of about 8,400 tenth metres and the extreme line visible has a wave length of about 9,200 tenth metres. At this point the closer ruled grating seemed to fail me, and I could not get beyond this point. It then struck me that the glass on which the lines were ruled might absorb the rays beyond (for I must explain that this grating was ruled on glass and silvered at the back) or that the red glass might absorb them. I then reverted to the first grating and adopted a different method of proceeding.

This was Fraunhofer's method, to which I was practically new—though I believe I have been credited (though inaccurately) with using it for other experiments—and after various attempts the following is an outline of the arrangement adopted. The slit was placed horizontally, a prism of 60° occupied a position at the end of the collimator next the lens, the reflection grating then received the rays and reflected them into the camera lens, the camera being tilted at an angle to make them fall on the sensitive plate. To show what sort of effect can be got I exhibit a photograph in which from the first to the fourth order of spectra were impressed on the same plate in a small camera. When this method was applied to the larger apparatus I got further into the ultra red than I had done before, and such a plate you see before you; the wave length of the last line of my group of lines visible on the plate is about 10,300 tenth metres. From indications on other plates I am inclined to think that we may get as far as 12,000 tenth metres, which it will be admitted is a tolerable distance to travel along the

invisible spectrum. In these ultra regions the lines are faint, but perfectly measurable under a moderate magnifying power, and I have therefore proposed to myself to make a map of the ultra red. For the purpose I propose to use the overlapping of the higher order of the spectrum over that used. By cutting off the red in one case and using half the length of the slit for one exposure, and then by cutting off the blue and using the remaining half of the slit for the other, we shall have one spectrum over the other. The wave lengths of the most refrangible rays are known, and since the dispersion of the higher order is double the one below it, the wave lengths of the latter can be accurately ascertained. When once a scale is obtained the greatest difficulty will have vanished.

Now as to the process. My object has been to weight the molecules of silver bromide that they may absorb the red rays. With ordinary silver bromide the film allows these very rays to pass through whilst a blue absorption takes place. In other words, my endeavour has been to find a heavier molecule of a sensitive salt, which shall answer to the swing of the waves of the red and ultra red rays. This I first accomplished, as I stated at the time, by adding resins to the silver salt and forming what I may call a bromo-resinate of silver. But I am happy to say that I have secured the same end by, I believe, doubling the molecule of the silver bromide. Now this doubling the molecule is a matter of manipulation more than of chemical knowledge, and I might describe the process in detail, as I have already done in papers I have published, and yet the double molecule would not be obtained unless careful manipulation was attended to—manipulation easy to follow when seen, but difficult to follow from any description. I should therefore prefer to teach practically anyone who is acquainted with silver bromide emulsion making, rather than allow him to be misled by what must be imperfect directions.

I exhibit two films, both prepared with sensitive emulsion, which is composed of exactly similar ingredients, viz. pure silver bromide. You will at once note the difference in colour of the light transmitted. The one which is sensitive to the red and ultra red is of a blue tint; the other is orange. You will see that the blue tint would appear to be due to a physical arrangement of the molecules, for if a part of the film be rubbed you will see that it changes to a ruddy tint, passing through an emerald green stage. We have it so on the screen. Whether it be right or wrong in regard to other matter, I am convinced that in silver bromide we have the possible existence of two sizes of molecules. In the blue film we have the presence of both; when only the larger size is present we shall have a compound which is very much more sensitive to the lower end of the spectrum than it is to the upper. Allow me to say that these views are not original except in so far as they are applied to this subject. Whether they are correct or not, they have formed a good working hypothesis which has led me to the results obtained.

Before closing I must refer to the comparative lengths of exposure required for these photographs. My impression is that at A the exposure required is about 25 times that required for G, in a fairly bright sun at midday; for the ultra red, as far as I have gone, I should say about 35 times. These are only approximations, but still will enable you to form some idea of the sensitiveness. I show you a photograph taken about 4 P.M. on March 18. The exposure was about 50 seconds. You will see that the red end is as strong as the blue, with the yellow much lacking in density. In other words, the yellow rays are nearly inactive.

The photograph which I showed you of the four orders of the spectrum was taken on April 3, at 2.30. It had an exposure of one minute and a half. The photograph in which the furthest band of lines was seen had an exposure of 12 minutes on the 8th, at 2.30. The slit was in this case closed as nearly as possible.

In conclusion, I have to remark that in a short time I hope to reduce the exposures considerably. In the course of some investigations, the results of which have just been communicated to the Royal Society, I found that the red rays could oxidise a photographic image as well as form it, and that in an oxidised state it was unable to be developed. If the tendency of the sensitive compound to become oxidised exceeded its tendency to become reduced, no image could be developed. By exposing *in vacuo* or in a nitrogen atmosphere I hope to eliminate altogether this oxidising effect, and so get firmer images.

I trust that this paper may be considered as the fulfilment of the promise implied in my preliminary note.

On the Duration of Meteor Showers. By R. P. Greg, Esq.

In the No. of the *Monthly Notices* of the Society for January 1878, Mr. Denning has a paper on "Suspected Secondary or Repetitive Outbursts from Radiant-points; and on the long Duration of Meteor Showers"; and Captain Tupman has appended some valuable remarks on Mr. Denning's paper.

1. In the case of a *second* outburst of the same supposed meteoric radiant, some two, three, or even four months after the first (supposing it to be a true case of double intersection of the same meteor orbit, say near its perihelion), then I imagine it could easily be shown that the apparent positions of the radiant, on each occasion, must necessarily lie at least in different quarters of the heavens; and it would be an almost impossible matter to decide whether the two radiants belonged to one and the same shower and orbit; unless indeed the individual meteors were wonderfully characteristic and similar in appearance.

2. With respect to the *duration* of meteor showers, though I

think Mr. Denning may rather exaggerate that element, my own experience would certainly favour the idea of a longer average duration for meteor showers having a very fairly decided fixed radiant than one could have supposed likely. In *not* a few cases the duration would even seem to endure for two or three months without any special intermission. But leaving out such extreme cases, as caused probably by two distinct showers overlapping each other as it were, let us consider the *average* duration of meteor showers, so far as can be judged from some of the principal Catalogues. In the late Professor Heis' latest Catalogue (1877) there are specified no less than 120 different meteor showers with their radiant positions for the six months July to December. The *average* duration I find for each is $20\frac{1}{2}$ days and the number of meteors recorded about 6,000, without any undue allowance for *Perseids* and *Leonids*, and there are 14 showers lasting over 34 days. The observations extend over a period of 25 years.

The average duration of 105 showers, 2,300 meteors, as deduced from Mr. Denning's reductions of the Italian observations, 1872, July–December, is 24 days. Mr. Denning's own observations, reduced from 2,170 meteors in 1876–7, give an average of 22 days.

In Dr. Schmidt's Catalogue there are 45 meteor showers with duration of about 30 days or more. In my own Catalogue, reduced from 2,000 meteors seen in England 1849–1867, the average duration for 40 showers for the whole year is 33 days (omitting 12 showers over 54 days, some of which are doubtless not really one shower).

I think we may therefore take it for granted, until at least proved to be incorrect, that the *average duration* of a meteor shower having a pretty constantly fixed radiant area, say of from 3°–8° in diameter, is not less than *three weeks*. Now, as some of these meteor showers only endure for *one* or *two* days, it is not unreasonable to assume for a *maximum* a duration of even *six weeks*; that is to say, meteor showers, so far as ascertained, endure from one to at least 40 days, giving in most cases a pretty fixed radiant in the heavens. There are not an inconsiderable number of cases in which the duration would appear to be even as great as 50 or 60 days, but this would appear to be so surprising that further proof and long continued nightly watching and recording is requisite to confirm it. The *Perseids* belong to a shower having a strong *maximum*, as is well known, about August 10, but it is perhaps not equally well known that this shower feebly commences about July 24 and continues until about August 17, ending much more suddenly than it commences.

The *Leonids* last for a few days with a strong maximum of only a few hours; the *Andromedids* for not more than half-a-day.

When, as we have shown, it may appear highly probable that the *average* duration of a meteor shower (of which at least 200 are now known whose orbits are intersected by the Earth) is

about *three weeks*, it must stand to reason that a considerable number may be expected to show a duration of at least five or six weeks, if not very occasionally more.

Captain Tupman has clearly pointed out the special conditions required to produce a nearly fixed meteor radiant for several weeks, viz. "The meteor orbit must nearly coincide with the plane of the ecliptic, the perihelion distance of the central position be a little less than unity, and the motion direct. The position would be 90° before the Sun at the middle time."

I am inclined, however, to think that another reason, which may assist in explaining the apparent difficulty of a rather frequent *long* duration with fixity of radiant, may be found in the supposition that the zone or ring of meteors belonging to a majority of showers may be a good deal wider or more diffuse than hitherto supposed, possessing a width in fact of several millions of miles, and this at the same time not improbably coincident with a considerable degree of parallelism in the direction of the orbits of the Earth and meteors at the time.

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*On the Solution by Trial of Lambert's Theorem, in Olbers' Method for the Computation of Parabolic Orbits.** By R. H. M. Bosanquet, Fellow of St. John's College, Oxford.

In all the different forms of the method of Olbers for the computation of parabolic orbits there is a certain step, depending in principle on the finding by trial of values of the heliocentric radii vectores of the comet which satisfy the condition given by the application of Lambert's Theorem to the observed times.

In the Appendix to Olbers' treatise, at pp. 68–71 of Encke's edition (1864), a method is given by which, from any assumption as to the value of the chord joining the first and third places of the comet, or of the sum of the corresponding geocentric radii vectores, more accurate values can be deduced by Lambert's theorem. This discussion contains the germs of the principal subsequent improvements of the method; and the form given to Lambert's theorem in the Note (Encke's *Olbers*, p. 70) is that on which the present discussion will be based.

In the Gaussian modification of Olbers' method, a plan is commonly adopted which is in principle the same as that recommended by Olbers in the place cited. It consists in assuming a value of u, for which Encke has given the formula

$$\left\{\left(\frac{t''-t}{4\mathrm{I}}\right)^2-\mathrm{A}^2\right\}^{\frac{1}{2}}.$$

* A paper on this subject was written by the author some years ago, and appeared in the *Astronomische Nachrichten* in 1872. On reading it over the author thinks that the method may still be worth the attention of the Society. The investigation is substantially unaltered, but the discussion of the example has been revised.

determining r and r'' (heliocentric radii vectores) on this hypothesis; thence k^2 (square of chord) by the auxiliary table of Encke, and then u again from the equation

$$k^2 = u^2 + A^2.$$

The value of u so obtained is generally better than that originally assumed, just as in the original rules of Olbers the value of k^2 resulting was better than that originally assumed.

So far as I know, however, the conditions of this improvement have not been directly investigated; and it has seemed to me worth while to examine under what conditions and to what extent this improvement takes place. I have been thus led to the following simple method, by which it would appear that, after calculation of two hypotheses as to the value of u and of a certain formula, a value of u may be deduced nearly as accurate as five-place calculations will admit.

Let the value of u first assumed be called u_o, and let that obtained after calculation of r, r'', and k^2 be u_1. Let the true value of u be called u, and let

$$u_o = u + \delta u_o, \qquad u_1 = u + \delta u_1.$$

If we then seek a relation between δu_o and δu_1, then by means of this relation and the equation

$$u_o - u_1 = \delta u_o - \delta u_1,$$

the values δu_o, δu_1, can be determined, and a value of u deduced closely approximating to the true value required.

In fact, if we combine a result of the form

$$\delta u_1 = -F\,\delta u_o,$$

with

$$u_o - u_1 = \delta u_o - \delta u_1,$$

we get

$$\delta u_o - \delta u_1 = -\left(\frac{1}{F} + 1\right)\delta u_1 = u_o - u_1,$$

and therefore

$$\delta u_1 = -(u_o - u_1)\frac{F}{F+1};$$

whence

$$u = u_1 - \delta u_1$$

$$= u_1 + (u_o - u_1)\frac{F}{F+1},$$

which is the expression for the improved value of u.

To deduce an expression for F:—

Putting $u + \delta u_o$ for u in the Gaussian equation

$$r^2 = \left(\frac{u+c}{b}\right)^2 + B^2,$$

we have for the corresponding error in r

$$\delta r = \delta u_o \frac{u+c}{rb^2},$$

neglecting the squares of the errors; similarly

$$\delta r'' = \delta u_o \frac{u+c''}{r''b''^2};$$

and if we write $r+r'' = \mathrm{S}$,

$$\delta \mathrm{S} = \delta u_o \left(\frac{u+c}{rb^2} + \frac{u+c''}{r''b''^2}\right). \qquad \text{(I)}$$

Now if in Lambert's theorem we put

$$\vartheta = \kappa\tau, \ (\log \kappa = 8{\cdot}5366114, \text{ Encke's } \textit{Olbers}, \text{ p. } 251),$$

where τ is the interval of time between the first and last observations, we can expand k^2, the square of the chord joining the first and last positions of the comet, by means of Lambert's theorem, in terms of ϑ and S. Thus:—

$$k^2 = \frac{\vartheta^2}{\mathrm{S}} + \frac{\vartheta^4}{12\mathrm{S}^4} + \frac{\vartheta^6}{36\mathrm{S}^7} + \ldots.$$

Encke's *Olbers*, p. 70, Note.

If we write $\eta = \dfrac{\vartheta}{\mathrm{S}^{\frac{3}{2}}}$, the series becomes

$$k^2 = \frac{\vartheta^2}{\mathrm{S}}\left\{1 + \frac{\eta^2}{12} + \frac{\eta^4}{36} + \ldots\right\},$$

where the series within the brackets is μ^2; $\log \mu$ being the quantity which is given with η as argument in Encke's table for the solution of Lambert's theorem.*

In the series for k^2 write $k^2+\delta(k^2)$ for k^2, and $\mathrm{S}+d\mathrm{S}$ for S; then

$$\delta(k^2) = -\frac{\vartheta^2}{\mathrm{S}^2}\left(1 + \frac{\vartheta^2}{\mathrm{S}^3}\right)\delta\mathrm{S}$$

$$= -\eta^2 \mathrm{S}\, d\mathrm{S}, \qquad \text{(II)}$$

omitting the terms of the expansion after the first, which may be done for approximate purposes.

Again the equation $k^2 = u_1^2 + \mathrm{A}^2$ gives

$$\delta(k^2) = 2u\delta u_1. \qquad \text{(III)}$$

* The expression $k = \dfrac{\vartheta}{\mathrm{S}^{\frac{3}{2}}}\mu$, on which Encke's table is founded, is generally proved in the books by a trigonometrical transformation. The present method is preferable when once the series for k^2 is regarded as known. This series occurs in Olbers' work, and it looks very much as if Encke's table had been originally formed in this way.

Combining I. II. III., we find

$$\delta u_1 = -\delta u_o \frac{S\eta^2}{2u}\left(\frac{u+c}{rb^2}+\frac{u+c''}{r''b''^2}\right);$$

or if

$$\delta u_1 = -F\,\delta u_o,$$

$$F = \frac{S\eta^2}{2u}\left(\frac{u+c}{rb^2}+\frac{u+c''}{r''b''^2}\right);$$

and we have the following conclusions :—

(1) The true value of u always lies between u_o and u_1 ;

(2) $u = u_1 + (u_o - u_1)\dfrac{F}{F+1}$ will be an approximate value of u.

The convenience of the process will be best illustrated by an example. That which I have selected is worked in different ways, as an example of the various methods, in Klinkerfues' *Theoretische Astronomie*, 1871, pp. 95–119. The number of successive hypotheses required to attain a solution is considerable, according to all the methods given.

I shall now give the computation of one hypothesis, and its improvement by means of the function F. The calculation of a second hypothesis, founded on the improved value, and the improvement of the value found in the second calculation are sufficient for accuracy to five places ; but as the second calculation is similar to the first, I give only its results. The further proceedings do not fall within the scope of this communication.

(Throughout the calculations, G. L. stands for "Gaussian logarithm"—*i.e.* addition or subtraction logarithm.)

Comet II. of 1813 :—

$t = 7{\cdot}55002$ April 1813.

$t' = 14{\cdot}54694$

$t'' = 21{\cdot}59931$

λ =	271°	16′	38″	β = 29°	2′ 0″
λ' =	266	27	22	β' = 22	52 18
λ'' =	256	48	8	β'' = 9	53 12

$\odot$ =	17	47	41	log R =	0·00091
$\odot'$ =	24	38	45	log R′ =	0·00175
$\odot''$ =	31	31	25	log R″ =	0·00260

Whence we have for the Gaussian quantities

log M =	9·75799	log B =	9·98706
G =	113° 43′ 57″	log B″ =	9·86038

$\log g = 9\ 38029$	$\log b = 9{\cdot}75645$
$H = 109°\ 5'\ 49''$	$\log b'' = 0{\cdot}05028$
$\zeta = 44°\ 13'\ 9''$	$g \cos \phi = 0{\cdot}17147$
$\log h = 9{\cdot}81477$	$c = 0{\cdot}31365$
$\log A = 9{\cdot}22527$	$c'' = 0{\cdot}95443$

Assume for first hypothesis $u_0 = 0{\cdot}3$ (Encke's preliminary rule gives 0·2984). Then we have in the usual way

$u_0 + c = 0{\cdot}61365$	$u_0 + c'' = 1{\cdot}25443$
$\log (u_0 + c) = 9{\cdot}78793$	$\log (u_0 + c'') = 0{\cdot}09844$
$\log \frac{1}{b} = 0{\cdot}24355$	$\log \frac{1}{b''} = 9{\cdot}94972$
$0{\cdot}03148$	$0{\cdot}04816$
$0{\cdot}06296$	$0{\cdot}09632$
$\log B^2 = 9{\cdot}97412$	$\log B''^2 = 9{\cdot}72076$
Diff. $0{\cdot}08884$	Diff. $0{\cdot}37556$
G. L. $0{\cdot}34772$	G. L. $0{\cdot}52820$
$\log r^2 = 0{\cdot}32184$	$\log r''^2 = 0{\cdot}24896$
$\log r = 0{\cdot}16092$	$\log r'' = 0{\cdot}12448$
$\log r'' = 0{\cdot}12448$	
Diff. $0{\cdot}03644$	
G. L. $0{\cdot}31963$	
$\log (r + r'') = 0{\cdot}44411 = \log S$	
$\log S^{\frac{1}{2}} = 0{\cdot}22206$	$\log \kappa = 8{\cdot}53661$
$\log \kappa (t'' - t) = 9{\cdot}68426$	$\log (t'' - t) = 1{\cdot}14765$
$9{\cdot}46220$	$9{\cdot}68426$
$\log \mu = 0{\cdot}00020$	$\log S^{\frac{3}{2}} = 0{\cdot}66618$
$\log k = 9{\cdot}46240$	$\log \eta = 9{\cdot}01808$
$\log k^2 = 8{\cdot}92480$	$\eta = 0{\cdot}10435$
$\log A^2 = 8{\cdot}45054$	$\log \mu = 0{\cdot}00020$
Diff. $0{\cdot}47426$	
G. L. $0{\cdot}29672$	
$\log u_1^2 = 8{\cdot}74726$	
$\log u_1 = 9{\cdot}37363$	$u_1 = 0\ 23639$

And since u_o was 0·3,

$$u_o - u_1 = 0{\cdot}06361$$
$$\log (u_o - u_1) = 8{\cdot}80353.$$

We proceed to calculate,

$$F = \frac{S\eta^2}{2u}\left(\frac{u+c}{rb^2} + \frac{u+c''}{rb''^2}\right),$$

noticing that all the logs we require are already on the paper.

$\log \frac{u+c}{b} = 0{\cdot}03148$	$\log \frac{u+c''}{b''} = 0{\cdot}04816$
$\log \frac{1}{b} = 0{\cdot}24355$	$\log \frac{1}{b''} = 9{\cdot}94972$
0·27503	9·99788
$\log r = 0{\cdot}16092$	$\log r'' = 0{\cdot}12448$
0·11411	9·87340
9·87340	
Diff. 0·24071	
G. L. 0·43785	
0·31125	
$\log \eta^2 = 8{\cdot}03616$	$\log u_1 = 9{\cdot}37363$
$\log S = 0{\cdot}44411$	$\log z = 0{\cdot}30103$
8·79152	9·67466
9·67466	
9·11686 = log F	
G. L. 0·05341 = log (F + 1)	
$\log \frac{F}{F+1} = 9{\cdot}06345$	$u_1 = 0{\cdot}23639$
$\log (u_o - u_1) = 8{\cdot}80353$	0·007362
7·86698	$U_1 = 0{\cdot}24375$
$(u_o - u_1)\frac{F}{F+1} = 0{\cdot}0073618$	

The ultimate value of u on the assumed data is $u = 0{\cdot}24384$, according to my repeated verifications. (Klinkerfues states that it is 0·24389; this difference may possibly arise from some further corrections being admitted in the data). Consequently by this simple calculation, based on the results of one

ordinary computation, we have arrived within one unit in the fourth place: a far more expeditious approximation than that given by any process I have found in the books.

It will always be necessary to make a second computation for the purpose of a check. With reference to this we may observe that careful five-place work is well repaid in the second computation, though not in the first. But for the second value of F three places are sufficient, though we shall obtain five for illustration; in fact, the influence of the exactness of F in the second computation is so small that the result in this example is the same whether we recompute F or not. Proceeding in exactly the same manner as before, but employing $U_1 = 0{\cdot}24375$ instead of $u_o = 0{\cdot}3$, we find

$$u_2 = 0{\cdot}24385$$

and therefore

$$U_1 - u_2 = -{\cdot}00010$$

Recomputing F, we find

$$\begin{aligned} \log. F &= 9{\cdot}12483 \\ \text{G. L.} = \log (F+1) &= 0{\cdot}05435 \\ & \overline{\qquad\qquad} \\ & 9{\cdot}07048 = \log \frac{F}{F+1} \\ \text{Num. log } (U_1 - u_2) &= 6{\cdot}00000 \\ & \overline{\qquad\qquad} \\ & 5{\cdot}07048 \end{aligned}$$

and therefore

$$(U_1 - u_2)\frac{F}{F+1} = -{\cdot}000012$$

and to five places

$$U_2 = 0{\cdot}24384.$$

A recalculation gave for u_3 the same value to the last place; consequently the performance of two complete calculations in the manner indicated above has here been sufficient for five-place purposes.

The successive results are now placed together.

	Corrections given by present method
$u_o = 0{\cdot}3$	
$u_1 = 0{\cdot}23639$	$+{\cdot}00736$
$U_1 = 0{\cdot}24375$	
$u_2 = 0{\cdot}24385$	$-{\cdot}00001$
$U_2 = 0{\cdot}24384$	

I may notice briefly that a useful check is given by the first

deduction from our investigation (p. 356), viz. that the true value of u lies between u_0 and u_1, the initial and final values of one of the ordinary calculations, as exhibited at p. 359. For instance, in the present case, the final number (U_2) lies between both pairs, $u_0 - u_1$, and $U_1 - u_2$.

But the value of u given by Klinkerfues (p. 97) as that which would be obtained, "setzen wir die Versuche bis zur grössten Schärfe fort," is 0·24389, which falls beyond the limits $U_1 - u_2$ by four units of the last place. Here certainly either my theory was wrong, or my computation of $U_1 - u_2$ was wrong, or Klinkerfues was wrong. I believe, after a good deal of troublesome verification, that Klinkerfues' value is wrong upon the data assumed.

I think there can be no question that the present method gets over one of the most troublesome and unsatisfactory parts of the computation of parabolic orbits better than any method in common use.

Observations of the Companion of α Canis Majoris with the Transit Circle of the U.S. Naval Observatory. By J. R. Eastman, Esq.

Date.	Object.	Seconds of Transit.	Micrometer Readings.	Δα	Δδ	p	s
		s		s	r	°	″
March 20	α *Canis Majoris*	11·362	33·070	0·622	0·502	49·3	11·8
	Companion	11·984	32·568				
,, 22	α *Canis Majoris*	10·965	33·235	0·631	0·415	55·0	11·1
	Companion	11·596	32·820				
,, 23	α *Canis Majoris*	10·910	33·235	0·644	0·420	55·2	11·3
	Companion	11·554	32·815				

One revolution of the micrometer screw = 15″·312.

The transit of α *Canis Majoris* was observed over *six* threads and the companion over *five* threads. On March 20 both stars were unusually steady, and the observations of the difference of declination were much better than on the other nights.

Of course these observations have but little value in fixing the position of the companion, and are given only to show that it *may* be observed on a meridian instrument of 8·5 inches aperture.

Naval Observatory, Washington,
March 25, 1878.

English Observations of the Satellites of Mars, 1877.

The following communications relating to the English observations of the satellites of *Mars* are those referred to in the Report of the Council at the general meeting of February last, see p. 209.

Observations made at the Royal Observatory, Greenwich.

Outer Satellite.

Date.	Sidereal Time.	Position Angle.	Distance.		Observer.	Remarks.
	h m	h m	°	Radii.	"	
Sept. 5	9 33	20 33	270		60 ±	M.	Rough estimation.
Sept. 5	9 50	20 50	240	4	or 59	M.	Estimation.
Sept. 5	10 33	21 33	230	3¼	or 48	M.	Estimation.
Sept. 5	11 0	22 0	225 ±	4 ±	or 59 ±	W.C.	Rough estimation.
Sept. 19	12 21	0 17	190° to 195°	2	or 28	M.	Very rough est.
Sept. 28	10 36	23 7	110° to 115°	2½	or 33	M.	Estimation.
Sept. 29	8 55	21 30	248° 31′	80″·4		M.	Measure.
Sept. 29	9 10	21 45	255	6	or 77	M.	Estimation.

Inner Satellite.

Sept. 28	10 21	22 52	280° to 285°	2¼	or 29	M.	Estimation.
Sept. 28	10 41	23 12	285	2½	or 33	M.	Estimation.

In reference to these observations Mr. Christie writes:—

1. My first view of the outer satellite (on August 31, 12^h) was obtained when I was ignorant of its calculated position, being in fact under the impression that it ought to be on the other side of the planet. The other observations were made with a general knowledge of the time of E. or W. elongation, but without any Ephemeris of position angles and distances, so that the observer did not know whether the satellite would be N. or S. of *Mars*.

2. We are quite satisfied of the existence of objects seen distinctly in the approximate positions noted. The motion of the object seen on September 5 for several hours was sufficient to prove it to be a satellite, as it approached *Mars* somewhat, whilst if it had been a star the planet would have approached it by 60″ or 70″ in one and a half hours.* In the case of the inner satellite, the motion was slightly *from* the planet in the course of 20^m, whilst for a star there would have been an *approach* of 6″. No red colour was noticed in the outer satellite. The planet was wholly or partly hidden when observing the satellites. The outer one was well seen when fully half the disk of *Mars* was uncovered.

* With *increase* of position-angle up to 360°, about.

Estimated Positions of the Satellites, as observed at the University Observatory, Oxford.

The Outer.				The Inner.			
Date. 1877.	Sidereal Time, Oxford. h m	Position Angle. °	Distance. ″	Date. 1877.	Sidereal Time, Oxford. h m	Position Angle. °	Distance. ″
Sept. 8	23 0	69	80				
Oct. 2	21 31	80	80	Oct. 2	21 40	80	32
Oct. 4	22 5	305	61·5				
Nov. 1	22 30	225	30±				

In reference to these Professor Pritchard writes that no Ephemeris of the Satellites was used or known at the Observatory.

Outer Satellite of Mars.

Dr. Wentworth Erck writes:

That on September 3, at $23^h\ 0^m$ G.S.T., he most distinctly saw the satellite at position 64°, and continued to watch it at short intervals for a period of two hours, during which time it had moved to position 55°.

That on September 8, at $22^h\ 35^m$ G.S.T., he again saw the satellite at position 78°.

That at this date no Ephemeris of the satellite had been published to his knowledge, and that the first Ephemeris known to him was that in *Nature* of September 13, which had not reached his hands on September 15, on which day he again kept the satellite under observation for a period of nearly four hours, during which the positions were as follows:—

	h	m		°
Sept. 15	21	10	G.S.T.	Pos. 257
„	23	30	„	„ 243
16	1	0	„	„ 238

That again he kept the satellite under observation from September 25, $22^h\ 20^m$ G.S.T., when the position was 264°, to September 26, $0^h\ 35^m$, when the position was 255°.

That he again saw the satellite on September 27, $23^h\ 0^m$, at position 69°, and yet again for the last time on October 2, at 24 hours, when the position was 64°; but it had then become so faint that he had exceeding difficulty in seeing it at all.*

That comparison of the satellite with two stars seen at different times, in the field along with the satellite and in nearly similar positions with regard to the planet, enabled him subsequently to estimate the diameter of the satellite at 13·6 miles, or $\frac{1}{300}$th of the diameter of the planet, assuming the planet and the satellite to reflect light alike.

This satellite was more conspicuous than *Enceladus*, and was on one occasion seen distinctly and steadily with an equilateral triangular aperture whose side was 6 inches.

* The satellite was never seen at much less than two diameters of the planet from the limb.

The inner satellite was never seen; but unfortunately during the first half of September, that is to say during the most favourable period, despair of success with the inner satellite concentrated attention on the outer one.

The observations were made with that $7\frac{1}{2}$-inch object glass which Alvan Clarke first sent the late Mr. Dawes. At all times an hexagonal aperture was employed, with single lenses as eye-piece, giving powers of 250 and 300.

Mr. H. C. Key writes:

"I first saw the outer satellite of *Mars* on the night of September 19, at about $9^h\ 30^m$, in the position assigned in the Ephemeris published in *Nature*. I used an 18-inch silvered glass equatoreal Newtonian. With an ordinary negative eye-piece, and a bar in it hiding the planet, I could hardly do more than glimpse the satellite occasionally, owing to the scattered light from *Mars*; but with a concave eye lens giving nearly the same power, 180, I could see the satellite pretty distinctly notwithstanding the brilliancy of the primary. Two or three nights afterwards—I was prevented making a note at the time as several friends were with me—I again saw the satellite with the same eye-piece, having myself calculated its approximate position in the absence of an Ephemeris. I have no doubt whatever in my own mind that the object was a satellite of *Mars*. I looked long and carefully for the inner satellite, but failed to find it.

"Were the opportunity to come over again, I have little doubt that, with a diminished aperture on the telescope—12 inches instead of 18—and a better prepared method of hiding the primary, I should be able to measure the distance and position of this satellite without much difficulty.

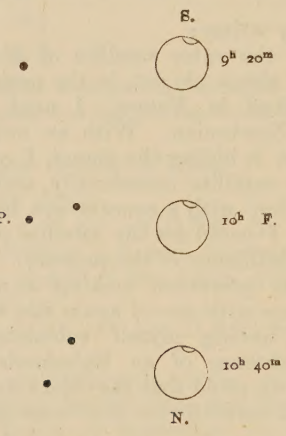
Mr. Brett writes:

"I enclose a tracing from the entry in my observing book, which I think comprises all the data I can offer as regards distance and position angle, being only eye estimates, and their accuracy being marred by the bar in the field, which was found necessary in order to conceal the disk of the planet. My bar, however (a minute slip of paper), was partially transparent, and enabled me to judge the distance in semi-diameters with tolerable accuracy.

"You will notice that the recorded observations extend over one hour and twenty minutes, but that only the outer satellite persists through the whole interval. The righthand one of the dots marked in my diagrams was only perceived a little before 10^h, and as its place did not at all accord with the ephemeris of the inner satellite as supplied to me, I have nothing to remark about it, except that, with equally good definition, it was not visible at $9^h\ 20^m$, but I thought it too *far* from the planet for the inner satellite (?).

"As regards the outer, the observations were quite free from doubt in my own mind. The point of light was exceedingly minute, but was *not* lost to sight at any time during the interval named.

"I had sought for the satellites on many evenings previously in vain, but the plane mirror had had its silver a good deal worn off. Early in September I procured from Mr. Browning a new plane mirror, and likewise from Greenwich an Ephemeris.



The Ephemeris for September 15 gave greatest elongation of outer satellite at 9h. On that occasion my new plane mirror mounted, and on placing the telescope on *Mars* I very soon perceived the minute point marked in diagram.

"The second minute point was of about the same brightness as the first. Both died out of sight shortly before 11.h

"Aperture of telescope 9′·1, power 380 (by With-Browning.)"

Mr. Marth has compared the foregoing observed places of the outer satellite, and also those of Mr. Common given in the January No., with places computed from approximate elements deduced from some of Professor Asaph Hall's observations.

Greenwich Observations.

				Obs.			Comp.	
		h m		Pos. °	Dist. ″		Pos. °	Dist. ″
1877	Sept. 5	9 33	G.M.T.	270	60	est.	245·8	82·9
		9 50		240	59	,,	244·6	81·4
		10 33		230	48	,,	241·4	76·9
		11 0		225 ±	59	,,	239·1	73·3
	19	12 21		190° to 195°	28	,,	182·9	28·6
	28	10 36		110° to 115°	33	,,	118·9	34·9
	29	8 55		248° 31′	80·4	meas.	249·11	73·7
		9 10		255°	77	est.	248·0	72·8

Oxford Observations.

	h	m		Obs. Pos. °	Obs. Dist. "	Comp. Pos. °	Comp. Dist. "
Sept. 8	23	0	Oxf. Sid. T.	69	80	71·7	85·5
Oct. 2	21	31		80	80	86·3	64·1
4	22	5		305	61·5	253·0	72·3
Nov. 1	22	30		225	30 ±	?	

Dr. Erck's Observations.

	h	m		Obs. °	Comp. °
Sept. 3	23	0	G. Sid. T.	64	65·6
	? 1	0		55	55·6
8	22	35		78	74·2
15	21	10		257	256·5
	23	30		243	247·2
16	1	0		238	267·1
25	22	20		264	266·3
26	0	35		255	322·4
27	23	0		69	72·9
Oct. 2	0			64	76·3

Mr. Brett's Diagrams.

Sept. 15	9	20	G.M.T.	?	257·2	6·8 rad.
	10				254·5	6·9
	10	40			251·9	6·9

Mr. Common's Observations.

	h	m		Obs. °	′	Comp. °
Sept. 11	9	52	G.M.T.	270		291·3
15	10	15		253	48	253·53
	10	20		252	44	253·20
	10	41		251	2	251·83
	10	47		250		251·5
	10	50		250	40	251·24
	12	43		244	28	243·28
	12	51		240	40	242·62
	13	5		243		241·5
16	12	43		273		273·0
	12	47		269		272·5
	12	50		273	30	272·07
	12	57		270		271·2

	h	m	Obs. °		"	Comp. °	"
	13	5			63·2		62·8
	13	44	266	4		266·28	
	13	50			72·1		69·8
	13	58	263	4		264·99	
	14	0	263	36		264·81	
	14	5	264	8	75·6	264·38	71·9
21	8	34	62	8		32·93	
24	8	49	240	30		243·84	
	9	8	236	30		242·24	
28	11	23	111			131·5	
29	9	28	244	30		246·69	
	9	33	245	45		246·30	
	10	29	242			241·64	
	10	36	239	3		241·18	
30	10	28	269	30		271·51	
	10	38	267	30		270·60	
	10	52	267	0		268·93	
	11	0	266			268·14	

Note on the Table, p. 70 of Vol. II. Dun Echt Observatory Publications. By Lord Lindsay.

There is in the description of the table an ambiguity, which may be cleared up thus:

In taking out N from the tables the sign is to correspond to the sign of the cosine of the Hour Angle for Latitude North, and the opposite sign for Latitude South

The sign of M is immaterial, as Z is always positive.

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